beginning with the seed and germination. Dr. Cavers lays some stress on the second chapter, which is intended to impress a more thorough knowledge of the organic products in plants. Seeing that the real aim of students' courses is rather to teach general methods and provide training than to implant facts, tests for proteins and other complex substances are much less valuable than the more tangible experiments of a physical nature.

Except in this matter, there is no hesitation in recognising that the author presents a remarkably clear and informative series of experiments. There is always satisfaction in experiments requiring simple and natural material, as in the test of a living turnip with beetroot juice, but Dr. Cavers on the whole favours the view that there is a necessity for specially designed apparatus capable of yielding exact measurements, in which connection he directs attention to several instruments designed by Prof. Ganong. An appreciable amount of generally unknown detail is supplied in the life-history of Pellia and Funaria, and otherwise this section is no mere repetition of available information. Teachers will be well advised to consult the book before drafting their physiological courses, as they are tolerably certain to discover suggestions or new experiments.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* Nature. *No notice is taken of anonymous communications.*]

Heredity.

So long as naturalists persist in using ill-defined terms, the meaning of which they have not clearly thought out, the controversy about the inheritance of so-called "acquired characters" is bound to be sterile and interminable. If it be once granted that organisms are the product of the interaction of two sets of factors—the factors of the inheritance and the factors of the environment—it becomes obvious that not only every organism, but every "character" of an organism, must be the result of both sets of factors. And if by "character" we mean any such resulting structure or property as it appears to our senses, as we see it before us, then it becomes manifest that no character can be due wholly to inheritance or wholly to environment. The very words "acquired character" involve a fatal fallacy—suggesting as they do that one character may be more acquired than another. Since such wholly acquired characters do not exist, it is waste of time to discuss their possible inheritance.

Even Dr. Reid, in his letter in last week's Nature, does not entirely escape from this logical error when he uses the word inheritance for the transmission of acquirements (characters) in unicellular organisms. It is a return to the vague, popular use of the term which would inevitably lead us back into the old tangle of inconsistencies. The biologist may define inheritance as the transmission of hereditary factors —it is not ready-made characters which are inherited, but the factors which help to produce them. The transmission in a protozoon of the characters of its parent is no more inheritance in the strict biological sense than is the transmission of the eggshell and albumen from the fowl to the chick, or of money from father to son.

Variation may be caused by changes in the environment giving rise to "modification," or by changes in the inheritance (the totality of the hereditary factors) giving rise to "mutation." Changes in the inheritance are due to the rearrangement of, addition to, or subtraction from the factors of inheritance. Ultimately these changes must be referred to the environment, and it is only when something from the environment thus alters or enters into the inheritance that mutation can occur.

It follows that if certain observations seem to show that "acquired characters" are transmitted by true inheritance, either they must be capable of some other interpretation, or our premise that every organism is the resultant of two sets of factors must be wrong. No escape from this alternative seems possible.

The dogmatic tone of this letter will, I hope, be forgiven me, as it has been assumed merely for the sake of brevity. E. S. Goodrich.

Merton College, Oxford, March 1.

Mars and a Lunar Atmosphere.

In Nature, February 22, p. 565, reference is made to an interesting observation by Prof. Luther, of the Düsseldorf Observatory. The note states that he saw the half of the disc of Mars nearest the moon become green just before occultation on December 4, 1911, and he suggests that this may have been due to a lunar atmosphere. The time was 16h. 40m. (Greenwich mean time), and I notice that the moon was full at 14h. 52m. on December 5, so that, at the time of the observation, the unilluminated crescent of the moon towards the planet must have been extremely narrow, so that the illuminated part of the lunar disc must have been quite close to the planet.

Now no refracting telescope is perfectly achromatic, and as one of the residual colours is green, it seems to me possible that this colour may have been due to moonlight imperfectly achromatised. It may also be suggested that the reddish colour of Mars might lead to the focus of the telescope being different for the planet and the moon. Another suggestion is that the colour of the planet might give rise to a complementary tint.

Turning to the date of Prof. Luther's previous observation, October 16, 1902, I find that the moon was full on that very day, and this seemed to link the two observations together, both being associated with a nearly full moon.

But, to my surprise, I found, on consulting the Nautical Almanac table of occultations, that no occultation of Mars, or of any planet, is set down for October 16, 1902, and, on looking up the positions of the moon and of Mars, it is obvious that none could have occurred, as they were distant in R.A. by some nine hours. It is evident then that there is some mistake in the earlier date, unless it is meant to apply to some small stars in Pisces.

I observed with a refractor the disappearance occultation of Mars at the dark limb of a moon rather more than half-full in the early morning of January 29 this year, but saw no trace of any green colour on the disc of the planet.

C. T. Whitmell.

Hyde Park, Leeds, February 26.

The Teaching of Mathematics.

In an article entitled "The Teaching of Mathematics" in Nature of November 30, 1911, considerable space is devoted to a memorandum written by me for

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the Department of Public Instruction of New South Wales, to accompany and explain the programmes for the mathematical classes in the high schools recently established in this State. Regret is expressed by the writer of that article that New South Wales "has been frightened by difficulties which were bound to arise in a period of transition, into going back to the old methods instead of boldly remedying the evil by helping all teachers to get the spirit of the new methods."

The point at issue is the treatment of the fundamental theorems of congruence, parallels, and the angle-sum for a triangle, in the course of deductive geometry given to pupils in these high schools. In the programmes, as issued, the teachers are advised to follow Euclid's method (or something of the same nature) in these fundamental theorems. The Board of Education circular, from which we have ventured to differ in this particular alone, recommends that these results be obtained by induction and experiment.

It seems proper that your readers should be aware of the following facts:—

(i.) The course of geometry in question is not meant for children of twelve years of age and under, as the writer of your article seems to assume.

Pupils enter these schools after completing a full course of primary education. Their age at entrance varies from thirteen to thirteen and a half.

(ii.) Before entering the high schools they have had a full year's work at geometry. In this preliminary study the newer methods are fully employed; the results are obtained by induction and experiment, and a great part of what the Board of Education circular recommends is adopted. However, the box of mathematical instruments does not hold sway to the entire exclusion of theoretical work.

(iii.) Although it has been thought advisable to ask for some uniformity of treatment in these early theorems in the deductive course, when this stage is past the fullest amount of freedom is granted.

There is no doubt that experience will show that some modifications in the syllabus are necessary. Some of the points mentioned in your article had already been noted as requiring alteration, and the suggestions which it contains will certainly receive the careful attention of the proper authorities. But the decision with regard to the earlier stages of the geometry course was made only after the fullest consideration. For this reason it is to be regretted that it has been, to some extent at least, misunderstood by the writer of your article.

Sydney, January 10. H. S. CARSLAW.

IN spite of Prof. Carslaw's assurance that pupils on entering secondary schools have reached the age of thirteen or thirteen and a half and have had a full year's work at geometry, the writer of your article feels most strongly that it is extremely unwise to impose on them in their first year at the secondary school a logical treatment of the fundamental theorems of congruence and parallels. Anyone who has had much experience of teaching pupils of that age knows how difficult it is to teach this work and how little impression it makes except on a very small minority; on the other hand, if these theorems are frankly assumed (after the pupils thoroughly understand their meaning) the rest of the geometry usually done in secondary schools can be treated logically, and the vast majority of pupils will get a proper grasp of the ideas of logical geometry. In the latter case the foundations are broad and the structure is firm at every stage; if the fundamental theorems are treated logically, an attempt is made to build on a narrower base, but in the majority of cases the lower stories of the structure are insecure.

The writer of your article must still regret the attitude taken up by the New South Wales authorities on this point. R. Y. S.

The Isothermal Layer.

IN reading Dr. Evans's reply to my letter in NATURE of January 25 with regard to the isothermal layer, I was specially interested in his reference to radiation of heat from orbital interplanetary matter as a probable climatic factor, because in *Symon's Meteorological Magazine* of February, 1911, I suggested that the recurrence year after year of warm and cold periods, first directed attention to by the late Dr. Buchan, may be attributable to modifications in a screen of cosmic matter, such, for instance, as that from which the zodiacal light and the Gegenschein are reflected.

I mentioned in a later number of that magazine that my own observations of the light in tropical latitudes, extending over several years, conveyed to me the impression of a ring of cosmical bodies encircling the earth about the zodiac.

For evidence of the isothermal layer at the equator the report of Prof. Borson on the aërological expedition of the Royal Prussian Aëronautical Observatory to East Africa in 1908[1] may be quoted. In this report at least two instances are recorded of balloon ascents near the equator in which the isothermal was reached: on August 30, at a height of 17·3 km., when a temperature of −82·5° C. was registered, and slightly lower temperatures at higher elevations; and on September 5, at 15·4 km., temperature −70·3° C., slight inversions being registered at greater altitudes.

The greater height of the isothermal in equatorial regions may be due to strong convection currents, as Mr. W. H. Dines supposes,[2] even though the origin of the layer be attributable to reflected heat from interplanetary matter. CAMPBELL HEPWORTH.

2 Amherst Road, Ealing, W., February 18.

St. Elmo's Fire.

ON Thursday evening, February 22, about 9.20 p.m., whilst traversing a country road which crosses the head of Carr Wood, a well-wooded clough in the neighbourhood of Heywood, near Rochdale, I was fortunate enough to witness a most unique phenomenon. The road in question skirts a hill on the left-hand side, and the opposite side, at this particular place, overlooks a small plateau which runs along the edge of the clough.

During the day we had had much rain. The atmosphere was now very close and heavy, and everything was ominously silent, even the usual *breeze having disappeared.* Suddenly, without the slightest warning, there appeared an area of faint electric-blue light, almost circular in shape and about 70 yards in diameter, which covered the plateau. The edge of this area was not more than 10 yards from where the observations were made. The whole electric field seemed to be three or four feet above the ground-level, and was in a state of intense agitation. Within the general blue ground there appeared flashes of a more decided blue, very similar in character to forked lightning, but not nearly so distinct.

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[1] "Results of Investigations of the Royal Prussian Aeronautical Observatory at Lindenburg." Edited by the Director, Doctor Richard Assman.

[2] "The Vertical Temperature Distribution in the Atmosphere over England, and some remarks on the General and Local Circulation." By W. H. Dines, F.R.S., Phil. Trans. Royal Soc., Series A, 211, page 269.

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